

# THE DAPPLED WORLD

A Study  
of the  
Boundaries of Science

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NANCY CARTWRIGHT



# The Dappled World

## *A Study of the Boundaries of Science*

It is often supposed that the spectacular successes of our modern mathematical sciences support a lofty vision of a world completely ordered by one elegant theory. In this book Nancy Cartwright argues to the contrary. When we draw our image of the world from the way modern science works – as empiricism teaches us we should – we end up with a world where some features are precisely ordered, others are given to rough regularity and still others behave in their own diverse ways. This patchwork of laws makes sense when we realise that laws are very special productions of nature, requiring very special arrangements for their generation. Combining previously published and newly written essays on physics and economics, *The Dappled World* carries important philosophical consequences and offers serious lessons for both the natural and the social sciences.

Nancy Cartwright is Professor of Philosophy at the London School of Economics and Political Science and at the University of California, San Diego, a Fellow of the British Academy, and a MacArthur Fellow. She is the author of *How the Laws of Physics Lie* (1983), *Nature's Capacities and their Measurement* (1989), and *Otto Neurath: Philosophy Between Science and Politics*, co-authored with Jordi Cat, Lola Fleck and Thomas Uebel (1995).



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**To Emily and Sophie**





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## Acknowledgements

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This book is squarely in the tradition of the Stanford School and is deeply influenced by the philosophers of science I worked with there. It began with the pragmatism of Patrick Suppes and the kinds of views he articulated in his *Probabilistic Metaphysics*.<sup>1</sup> Then there was Ian Hacking, John Dupré, Peter Galison and, for one year, Margaret Morrison.

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<sup>1</sup> Suppes 1984.



## Introduction

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This book supposes that, as appearances suggest, we live in a dappled world, a world rich in different things, with different natures, behaving in different ways. The laws that describe this world are a patchwork, not a pyramid. They do not take after the simple, elegant and abstract structure of a system of axioms and theorems. Rather they look like – and steadfastly stick to looking like – science as we know it: apportioned into disciplines, apparently arbitrarily grown up; governing different sets of properties at different levels of abstraction; pockets of great precision; large parcels of qualitative maxims resisting precise formulation; erratic overlaps; here and there, once in a while, corners that line up, but mostly ragged edges; and always the cover of law just loosely attached to the jumbled world of material things. For all we know, most of what occurs in nature occurs by hap, subject to no law at all. What happens is more like an outcome of negotiation between domains than the logical consequence of a system of order. The dappled world is what, for the most part, comes naturally: regimented behaviour results from good engineering.

I shall focus on physics and economics, for these are both disciplines with imperialist tendencies: they repeatedly aspire to account for almost everything, the first in the natural world, the second in the social. Since at least the time of the Mechanical Philosophy, physicists have been busy at work on a theory of everything. For its part, contemporary economics provides models not just for the prices of the rights for off-shore oil drilling, where the market meets very nice conditions, but also for the effects of liberal abortion policies on teenage pregnancies, for whom we marry and when we divorce and for the rationale of political lobbies.

My belief in the dappled world is based in large part on the failures of these two disciplines to succeed in these aspirations. The disorder of nature is apparent. We need good arguments to back the universal rule of law. The successes of physics and the ‘self evidence’ of the governing principles in economics – the assumption that we act to maximise our own utilities, for example, or the fact that certain behaviours can be *proved* to be rational by game theory – are supposed to supply these

arguments. I think they show just the opposite. They show a world whose laws are plotted and pieced.

Consider physics first. I look particularly at quantum physics, because it is what many suppose – in some one or another of its various guises – to be the governor of all of matter.<sup>2</sup> I also look to some extent at classical physics, both classical mechanics and classical electromagnetic theory. For these have an even more firmly established claim to rule, though the bounds of their empire have contracted significantly since the pretensions of the seventeenth century Mechanical Philosophy or the hopes for an electromagnetic take-over at the end of the nineteenth century. And I look at the relations between them. Or rather, I look at a small handful of cases out of a vast array, and perhaps these are not even typical, for the relations among these theories are various and complicated and do not seem to fit any simple formulae.

The conventional story of scientific progress tells us that quantum physics has replaced classical physics. We have discovered that classical physics is false and quantum physics is, if not true, a far better approximation to the truth. But we all know that quantum physics has in no way replaced classical physics. We use both; which of the two we choose from one occasion to another depends on the kinds of problems we are trying to solve and the kinds of techniques we are master of. ‘Ah’, we are told, ‘that is only in practice. In principle everything we do in classical physics could be done, and done more accurately, in quantum physics.’ But I am an empiricist. I know no guide to principle except successful practice. And my studies of the most successful applications of quantum theory teach me that quantum physics works in only very specific kinds of situations that fit the very restricted set of models it can provide; and it has never performed at all well where classical physics works best.

This is how I have come to believe in the patchwork of law. Physics in its various branches works in pockets, primarily inside walls: the walls of a laboratory or the casing of a common battery or deep in a large thermos, walls within which the conditions can be arranged *just so*, to fit the well-confirmed and well-established models of the theory, that is, the models that have proved to be dependable and can be relied on to stay that way. Very

<sup>2</sup> Looked at from a different point of view it is superstring theory that makes the loudest claims right now to be a theory of everything. But superstring theory is not (yet?) a theory of the physical world, it is a speculation; and even its strongest advocates do not credit it with its own empirical content. Mathematics, they say in its defence, is the new laboratory site for physics. (See Galison forthcoming for a discussion of this.) Its claims to account for anything at all in the empirical world thus depend on the more pedestrian theories that it purports to be able to subsume; that is, a kind of ‘trickle down’ theory of universal governance must be assumed here. So, setting aside its own enormous internal problems, the empire of superstring theory hangs or falls with those of all the more long standing theories of physics that do provide detailed accounts of what happens in the world.

occasionally it works outside of walls as well, but these are always in my investigations cases where nature fortuitously resembles one of our special models without the enormous design and labour we must normally devote to making it do so.

Carl Menger thought that economics works in the same way.<sup>3</sup> Menger is one of the three economists credited with the notion of marginal utility. He is also famous for his attack on historical economics and his insistence that economics should be a proper science. By 'proper' science he meant one that uses precise concepts which have exact deductive relations among them. His paradigm was  $F = ma$ . In mechanics we do get this kind of exact relation, but at the cost of introducing abstract concepts like *force*, concepts whose relation to the world must be mediated by more concrete ones. These more concrete concepts, it turns out, are very specific in their form: the forms are given by the *interpretative models* of the theory, for example, two compact masses separated by a distance  $r$ , the linear harmonic oscillator, or the model for a charge moving in a uniform magnetic field. This ensures that 'force' has a very precise content. But it also means that it is severely limited in its range of application. For it can be attached to only those situations that can be represented by these highly specialised models. This is just what Menger said we should expect from economics: we can have concepts with exact deductive relations among them but those concepts will not be ones that readily represent arrangements found in 'full empirical reality'. This does not mean they never occur, but if they do it will be in very special circumstances.

Much of the theorising we do in economics right now goes in exactly the opposite direction to that recommended by Menger. But in the end this is no aid to any aspirations economics might have to take over the entire study of social and economic life. Economics and physics are both limited in where they govern, though the limitations have different origins. Contemporary economics uses not abstract or theoretical or newly invented concepts – like 'force' or 'energy' or 'electromagnetic field' – concepts partially defined by their deductive relations to other concepts, but rather very mundane concepts that are unmediated in their attachment to full empirical reality.<sup>4</sup> We study, for instance, the capacity of skill loss during unemployment to produce persistence in employment shocks, or the limited enforceability of debt contracts, or whether the effect of food shortages on famines is mediated by failures of food entitlement arrangements. Nevertheless we want our treatments to be rigorous and our conclusions to follow deductively. And the way you get deductivity when you do not have it in the concepts is to put enough of the right kind of structure into the model. That is the trick of building a model

<sup>3</sup> Menger 1883 [1963].

<sup>4</sup> Cf. Mäki 1996.

in contemporary economics: you have to figure out some circumstances that are constrained in just the right way that results can be derived deductively.

My point is that theories in physics and economics get into similar situations but by adopting opposite strategies. In both cases we can derive consequences rigorously only in highly stylised models. But in the case of physics that is because we are working with abstract concepts that have considerable deductive power but whose application is limited by the range of the concrete models that tie its abstract concepts to the world. In economics, by contrast, the concepts have a wide range of application but we can get deductive results only by locating them in special models.

Specifically, I shall defend three theses in this book:

- (1) The impressive empirical successes of our best physics theories may argue for the truth of these theories but not for their universality. Indeed, the contrary is the case. The very way in which physics is used to generate precise predictions shows what its limits are. The abstract theoretical concepts of high physics describe the world only via the models that interpret these concepts more concretely. So the laws of physics apply only where its models fit, and that, apparently, includes only a very limited range of circumstances. Economics too, though for almost opposite reasons, is confined to those very special situations that its models can represent, whether by good fortune or by good management.
- (2) Laws, where they do apply, hold only *ceteris paribus*. By 'laws' I mean descriptions of what regularly happens, whether regular associations or singular causings that occur with regularity, where we may, if we wish, allow counterfactual as well as actual regularities or add the proviso that the regularities in question must occur 'by necessity'. Laws hold as a consequence of the repeated, successful operation of what, I shall argue, is reasonably thought of as a *nomological machine*.
- (3) Our most wide-ranging scientific knowledge is not knowledge of laws but knowledge of the *natures* of things, knowledge that allows us to build new nomological machines never before seen giving rise to new laws never before dreamt of.

In considering my defence of these three theses it will help to understand the motives from which I approach the philosophy of science. I notice among my colleagues three very different impulses for studying science. It is a difference that comes out when we ask the question: why are even deeply philosophical historians of physics generally not interested in leading philosophers of physics and vice versa? Or what is the difference between the philosopher of biology John Dupré, with whom I have so much in common, and philosophers studying the mathematical structures of our most modern theories in



physics, from whom I differ so radically despite our shared interest in physics? These latter, I would say, are primarily interested in the world that science represents. They are interested, for instance, in the geometry of space and time. Their interest in science generally comes from their belief that understanding our most advanced scientific representations of the world is their best route to understanding that world itself. John Dupré too is interested in the world, but in the material, cultural and politico-economic world of day-to-day and historical life.<sup>5</sup> He is interested in science as it affects that life, in all the ways that it affects that life. Hence he is particularly interested in the politics of science, not primarily the little politics of laboratory life that shapes the internal details of the science but its big politics that builds bombs and human genomes. That kind of interest is different again from most historians and sociologists of science whose immediate object is science itself, but – unlike the philosophers who use our best science as a window to the world – science as it is practised, as a historical process.

My work falls somewhere in the midst of these three points of departure. My ultimate concern in studying science is with the day-to-day world where SQUIDS can be used to detect stroke victims and where life expectancy is calculated to vary by thirty-five years from one country to another. But the focus of my work is far narrower than that of Dupré: I look at the claims of science, at the possible effects of science as a body of knowledge, in order to see what we can achieve with this knowledge. This puts me much closer to the ‘internalist’ philosophers in the detail of treatment that I aim for in discussing the image science gives us of the world, but from a different motive. Mine is the motive of the social engineer. Ian Hacking distinguishes two significant aims for science: representing and intervening.<sup>6</sup> Most of my colleagues in philosophy are interested in representing, and not just those specialists whose concerns are to get straight the details of mathematical physics. Consider Bas van Fraassen, who begins with more traditional philosophical worries. Van Fraassen tells us that the foremost question in philosophy of science today is: how can the world be the way science says it is or represents it to be?<sup>7</sup> I am interested in intervening. So I begin from a different question: how can the world be changed by science to make it the way it should be?

The hero behind this book is Otto Neurath, social engineer of the short-lived Bavarian Republic and founding member of the Vienna Circle.<sup>8</sup> Neurath is well known among philosophers for his boat metaphor attacking the

<sup>5</sup> Cf. Dupré 1993.

<sup>6</sup> Hacking 1983.

<sup>7</sup> Van Fraassen 1991.

<sup>8</sup> See Cartwright *et al.* 1996.

foundational picture of knowledge. What is less well known is how Neurath went about doing philosophy. He was not concerned with philosophy as a subject in itself. Rather his philosophy was finely tuned to his concerns to change the world. Neurath was the Director of the Agency for Full Social Planning under all three socialist governments in Munich in 1919 and 1920 and he was a central figure in the housing movement and the movement for workers' education in Red Vienna throughout the Vienna Circle period. Neurath advocated what I have learned from my collaborator Thomas Uebel to call *the scientific attitude*. This is the attitude I try to adopt throughout this book. The scientific attitude shares a great deal with conventional empiricism. Most important is the requirement that it is the world around us, the messy, mottled world that we live in and that we wish to improve on, that is the object of our scientific pursuits, the subject of our scientific knowledge, and the tribunal of our scientific judgements. But it departs from a number of empiricisms by rejecting a host of philosophical constructs that are ill-supported by the mottled world in which we live, from Hume's impressions and the inert occurrent properties that replace them in contemporary philosophy to the pseudo-rationalist ideal of universal determinism.

Neurath was the spearhead of the Vienna Circle's Unity of Science Movement. But this does not put us at odds about the patchwork of law. For Neurath worked hard to get us to give up *our belief in the system*. 'The system' for Neurath is the one great scientific theory into which all the intelligible phenomena of nature can be fitted, a unique, complete and deductively closed set of precise statements. Neurath taught:

**'The' system is the great scientific lie.<sup>9</sup>**

That is what I aim to show in this book. My picture of the relations among the sciences is like Neurath's. Figure 0.1 shows what is often taken to be the standard Vienna Circle doctrine on unity of science: the laws and concepts of each scientific domain are reducible to those of a more fundamental domain, all arranged in a hierarchy, till we reach physics at the pinnacle. Figure 0.2 is Neurath's picture: the sciences are each tied, both in application and confirmation, to the same material world; their language is the shared language of space-time events. But beyond that there is no system, no fixed relations among them. The balloons can be tied together to co-operate in different ways and in different bundles when we need them to solve different problems. Their boundaries are flexible: they can be expanded or contracted; they can even come to cover some of the same territory. But they undoubtedly have boundaries. There is no universal cover of law.

<sup>9</sup> Neurath 1935, p. 116.

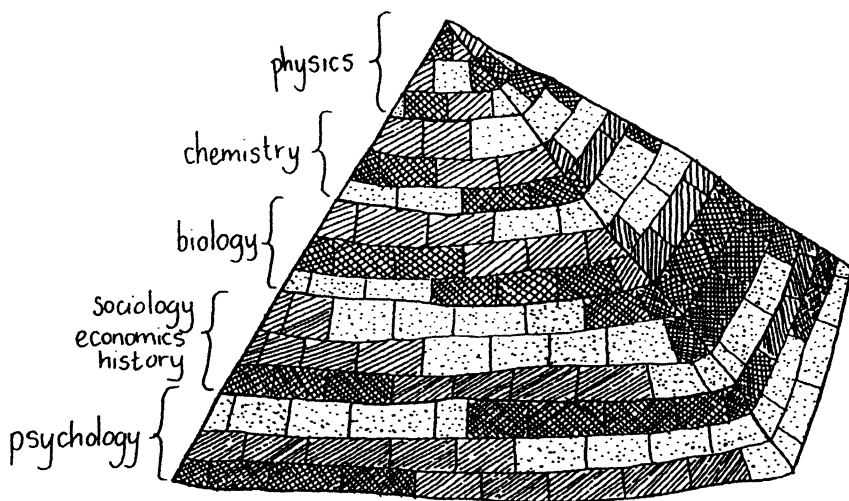


Figure 0.1 Pyramid. Source: Rachel Hacking.

The similarity of my views with Neurath's becomes clear when we turn to questions about the closure of theories. Consider theories in physics like classical Newtonian mechanics, quantum mechanics, quantum field theory, quantum electrodynamics, and Maxwell's theory of electromagnetism, or theories in economics like rational expectations or utility theory, Keynesian macroeconomics or Marxism. In these cases we have been marvellously successful in devising or discovering sets of concepts that have the features traditionally required for scientific knowledge. They are unambiguous: that is, there are clear criteria that determine when they obtain and when not. They are precise: they can not only be ordered as more or less; they can also be given quantitative mathematical representations with nice algebraic and topological features. They are non-modal: they do not refer to facts that involve possibility, impossibility or necessity, nor to ones that involve causality. Finally they have exact relations among themselves, generally expressed in equations or with a probability measure. Something like this last is what we usually mean when we talk of 'closure'.

Exactly what kind of closure do the concepts of our best theories in physics have? The scientific attitude matters here. The kind of closure that is supported by the powerful empirical successes of these theories, I shall argue, is of a narrowly restricted kind: so long as no factors relevant to the effect in question operate except ones that can be appropriately represented by the concepts of the theory, the theory can tell us, to a very high degree of approximation, what the effect will be. But what kinds of factors can be represented

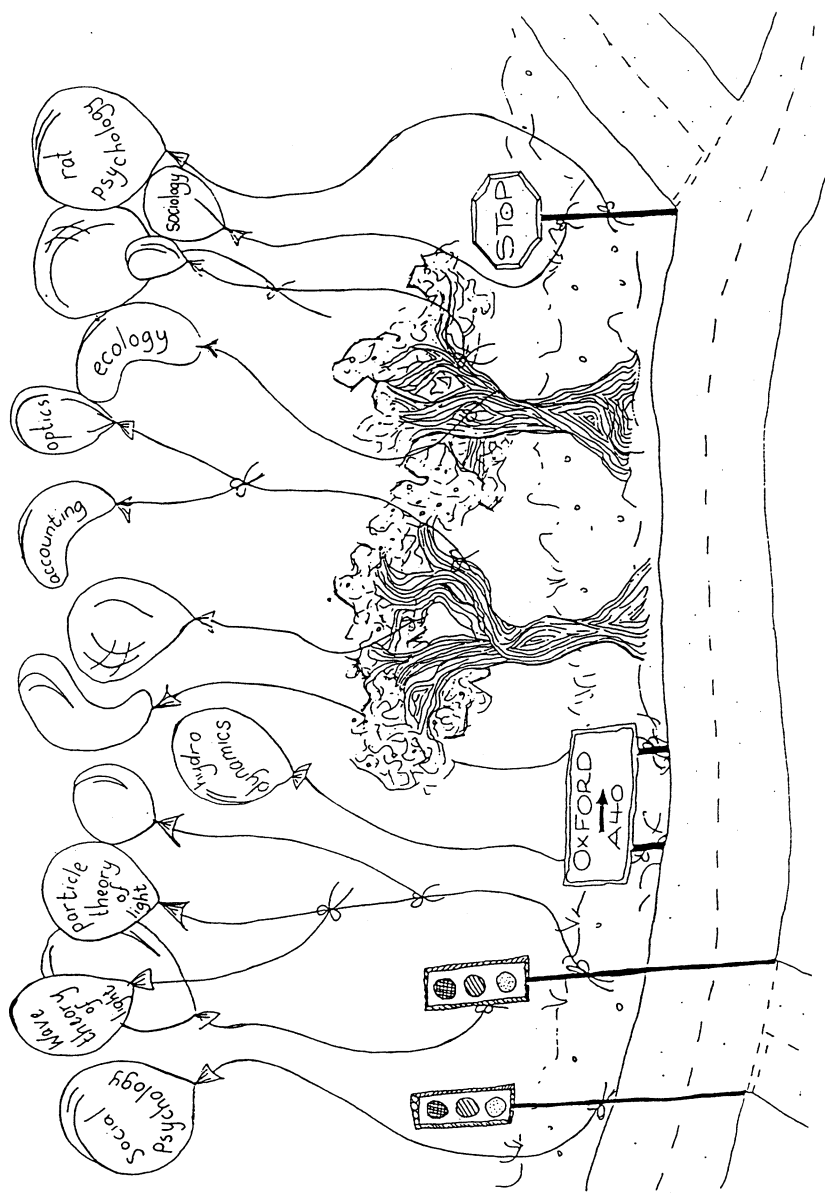


Figure 0.2 Source: Rachel Hacking.

by the concepts of the theory? The interpretative models of the theory provide the answer. And what kinds of interpretative models do we have? In answering this, I urge, we must adopt the scientific attitude: we must look to see what kinds of models our theories have and how they function, particularly how they function when our theories are most successful and we have most reason to believe in them. In this book I look at a number of cases which are exemplary of what I see when I study this question. It is primarily on the basis of studies like these that I conclude that even our best theories are severely limited in their scope. For, to all appearances, not many of the situations that occur naturally in our world fall under the concepts of these theories. That is why physics, though a powerful tool for predicting and changing the world, is a tool of limited utility.

This kind of consideration is characteristic of how I arrive at my image of the dappled world. I take seriously the realists' insistence that where we can use our science to make very precise predictions or to engineer very unnatural outcomes, there must be 'something right' about the claims and practices we employ. I will not in this book go into what the 'something right' could be, about which there is a vast philosophic literature. Rather I want to consider what image of the material world is most consistent with our experiences of it, including our impressive successes at understanding, predicting and manipulating it – but not excluding the limitations within which we find ourselves confined and the repeated failures to get it right that constitute far and away the bulk of normal scientific activity. The logic of the realists' claims is two-edged: if it is the impressive empirical successes of our premier scientific theories that are supposed to argue for their 'truth' (whatever is the favoured interpretation of this claim), then it is the theories as used to generate these empirical successes that we are justified in endorsing.

How do we use theory to understand and manipulate real concrete things – to model particular physical or socio-economic systems? How can we use the knowledge we have encoded in our theories to build a laser or to plan an economy? The core idea of all standard answers is the deductive-nomological account. This is an account that serves the belief in the one great scientific system, a system of a small set of well co-ordinated first principles, admitting a simple and elegant formulation, from which everything that occurs, or everything of a certain type or in a certain category that occurs, can be derived. But treatments of real systems are not deductive; nor are they approximately deductive, nor deductive with correction, nor plausibly approaching closer and closer to deductivity as our theories progress. And this is true even if we tailor our systems as much as possible to fit our theories, which is what we do when we want to get the best predictions possible. That is, it is not true even in the laboratory, as we learn from Peter

Galison, who argues that even the carefully controlled environments of a high-energy physics laboratory do not produce conclusions that can be counted as deductive consequences of physics theories.<sup>10</sup>

What should be put in place of the usual deductive-nomological story then? Surely there is no single account. Occasionally we can produce a treatment that relies on a single theory. Then the application of scientific knowledge can look close to deduction. But my thesis (1) teaches that these deductive accounts will work only in very special circumstances, circumstances that fit the models of the theory in just the right way. Even then, theses (2) and (3) imply that to cash out the *ceteris paribus* conditions of the laws, the deductions will need premises whose language lies outside the given theory; indeed premises that use modal concepts and are thus outside exact science altogether, as exact science is conventionally conceived.

Unfortunately, the special kinds of circumstances that fit the models of a single theory turn out to be hard to find and difficult to construct. More often we must combine both knowledge and technical know-how from a large number of different fields to produce a model that will agree well enough on the matters we are looking to predict, with the method of combination justified at best very locally. And a good deal of our knowledge, as thesis (3) argues, is not of laws but of natures. These tell us what *can* happen, not what will happen, and the step from possibility to actuality is a hypothesis to be tested or a bet to be hedged, not a conclusion to be credited because of its scientific lineage. The point is that the claims to knowledge we can defend by our impressive scientific successes do not argue for a unified world of universal order, but rather for a dappled world of mottled objects.

The conclusions I defend here are a development of those I argued for in *How the Laws of Physics Lie*. They invite the same responses. Realists tend towards universal order, insisting not only that the laws of our best sciences are true or are approaching truth but also that they are 'few in number', 'simple' and 'all-embracing'. *How the Laws of Physics Lie* maintained the opposite: the laws that are the best candidates for being literally true, whether very phenomenological laws or far more abstract ones, are numerous and diverse, complicated and limited in scope. My arguments, then as now, proceeded by looking at our most successful scientific practices. But what can be supported by arguments like these is, as realist-physicist Philip Allport points out, 'a far cry from establishing that such a realist account is impossible'.<sup>11</sup>

My answer to objections like Allport's was already given by David Hume a long time ago. Recall the structure of Hume's *Dialogues Concerning Natural*

<sup>10</sup> See Galison 1987 and 1997.

<sup>11</sup> Allport 1993, p. 254.

*Religion.* The project is natural religion: to establish the properties that God is supposed to have – omniscience, omnipotence, and benevolence – from the phenomena of the natural world. The stumbling block is evil. Demea and Cleanthes try to explain it away, with well-known arguments. Demea, for example, supposes ‘the present evil of phenomena, therefore, are rectified in other regions, and at some future period of existence’.<sup>12</sup> Philo replies:

I will allow that pain or misery in man is *compatible* with infinite power and goodness in the Deity . . . what are you advanced by all these concessions? A mere possible compatibility is not sufficient. You must *prove* these pure, unmixed and uncontrollable attributes from the present mixed and confused phenomena, and from these alone.<sup>13</sup>

Philo expands his argument:

[I]f a very limited intelligence whom we shall suppose utterly unacquainted with the universe were assured that it were the production of a very good, wise, and powerful being, however finite, he would, from his conjecture, form beforehand a very different notion of it from what we find it to be by experience; . . . supposing now that this person were brought into the world, still assured that it was the workmanship of such a divine and benevolent being, he might, perhaps, be surprised at the disappointment, but would never retract his former belief if founded on any very solid argument . . . But suppose, which is the real case with regard to man, that this creature is not antecedently convinced of a supreme intelligence, benevolent and powerful, but is left to gather such a belief from the appearance of things; this entirely alters the case, nor will he ever find any reason for such a conclusion.<sup>14</sup>

Philo is the self-avowed mystic. He believes in the wondrous characteristics of the deity but he takes it to be impossible to defend this belief by reason and evidence. Philo’s is not natural religion, but revealed.

Hume’s project was natural religion. My project is natural science. So too, I take it, is the project of Allport and others who join him in the belief in universal order. I agree that my illustrations of the piecemeal and bitty fashion in which our most successful sciences operate are ‘a far cry’ from showing that the system must be a great scientific lie. But I think we must approach natural science with at least as much of the scientific attitude as natural religion demands. Complication and limitation in the truest laws we have available are compatible with simplicity and universality in the unknown ultimate laws. But what is advanced by this concession? Just as we know a set of standard moves to handle the problem of evil, so too are we well rehearsed in the problem of unruliness in nature – and a good number of the replies have the same form in both discourses: the problem is not in nature but,

<sup>12</sup> Hume 1779 [1980], part X.

<sup>13</sup> Hume 1779 [1980], part X.

<sup>14</sup> Hume 1779 [1980], part XI.

rather, an artefact of our post-lapsarian frailties. I follow Philo in my reply: guarantee nothing *a priori*, and gather our beliefs about laws, if we must have them at all, from the appearance of things.

The particular theory in dispute between Allport and me was the Standard Model for fundamental particles. Let us allow, for the sake of argument, that the Standard Model can provide a good account of our experimental evidence, that is, that our experimental models for the fundamental fermions and bosons which concern Allport fit exactly into the Standard Model without any of the kinds of fudging or distortion that I worried about in *How the Laws of Physics Lie*. Since I have no quarrel with induction as a form of inference, I would be willing to take this as good evidence that the Standard Model is indeed true<sup>15</sup> of fundamental bosons and fermions – in situations relevantly similar to those of our experiments. Does that make it all-embracing? That depends on how much of the world is ‘relevantly similar’. And that is a matter for hard scientific investigation, not *a priori* metaphysics. That is the reason I am so concerned with the successes and failures of basic science in treating large varieties of situations differing as much as possible from our experimental arrangements.

As I have indicated, my investigations into how basic science works when it makes essential contributions to predicting and rebuilding the world suggest that even our best theories are severely limited in their scope: they apply only in situations that resemble their models, and in just the right way, where what constitutes a model is delineated by the theory itself. Classical mechanics, for instance, can deal with small compact masses, rigid rods and point charges; but it is not very successful with things that are floppy, elastic or fluid. Still, it should be clear from my discussion of natural religion, natural science and the scientific attitude that any conclusion we draw from this about the overall structure and extent of the laws of nature must be a guarded one. The dappled world that I describe is best supported by the evidence, but it is clearly not compelled by it.

Why then choose at all? Or, why not choose the risky option, the world of unity, simplicity and universality? If nothing further were at stake, I should not be particularly concerned about whether we believe in a ruly world or in an unruly one, for, not prizing the purity of our affirmations, I am not afraid that we might hold false beliefs. The problem is that our beliefs about the structure of the world go hand-in-hand with the methodologies we adopt to study it. The worry is not so much that we will adopt wrong images with which to represent the world, but rather that we will choose wrong tools with which to change it. We yearn for a better, cleaner, more orderly world than the one that, to all appearances, we inhabit. But it will not do to base our

<sup>15</sup> In the same sense in which we judge other general claims true or false, whatever sense that is.



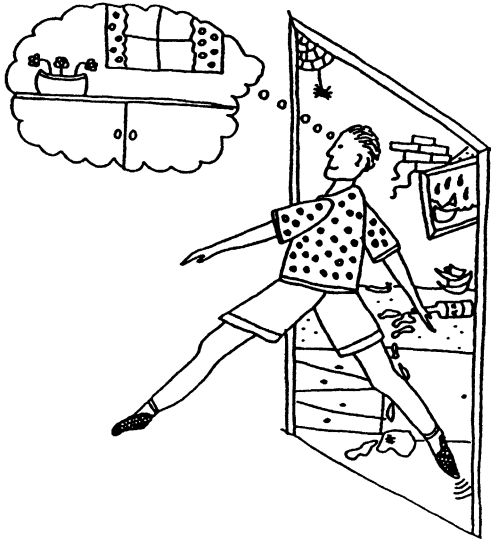


Figure 0.3 Source: Rachel Hacking.

methods on our wishes. We had better choose the most probable option and wherever possible hedge our bets.

A series of pictures drawn by Rachel Hacking illustrates my overall thesis. The figure in the pictures represents the philosopher or scientist who is yearning for unity and simplicity. In figure 0.3 he stands in the unruly and untidy world around him with a wistful dream of a better world elsewhere. Notice that our hero is about to make a leap. Figure 0.4 shows the ideal world he longs for. Figure 0.5 is the messy and ill-tended world that he really inhabits. Figure 0.6 is the key to my worries. It shows the disaster that our hero creates when he tries to remake the world he lives in following the model of his ideal image. Figure 0.7 is what I urge: it shows what can be accomplished if improvements are guided by what is possible not what is longed for.

For a concrete example of how belief in the unity of the world and the completeness of theory can lead to poor methodology we can turn to a discussion by Daniel Hausman of why evidence matters so little to economic theory.<sup>16</sup> Hausman notices that economists studying equilibrium theory make serious efforts to improve techniques for gathering and analysing market data, but they do not generally seek or take seriously other forms of data, particu-

<sup>16</sup> Hausman 1997.

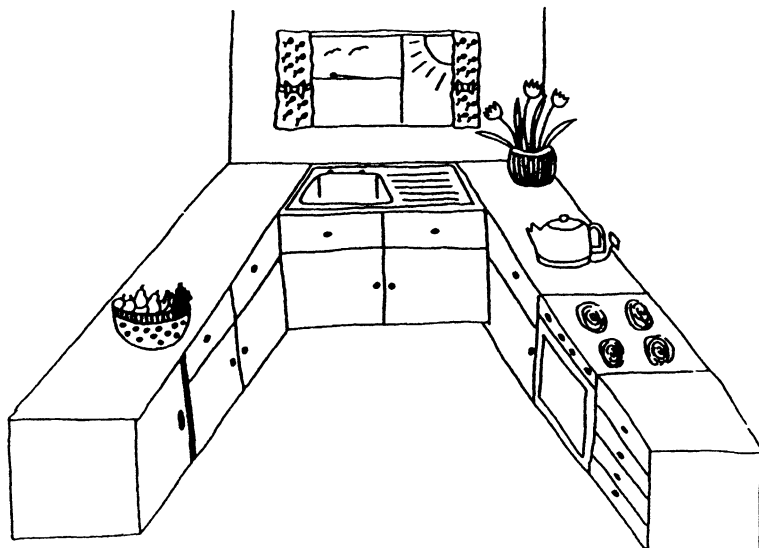


Figure 0.4 Source: Rachel Hacking.

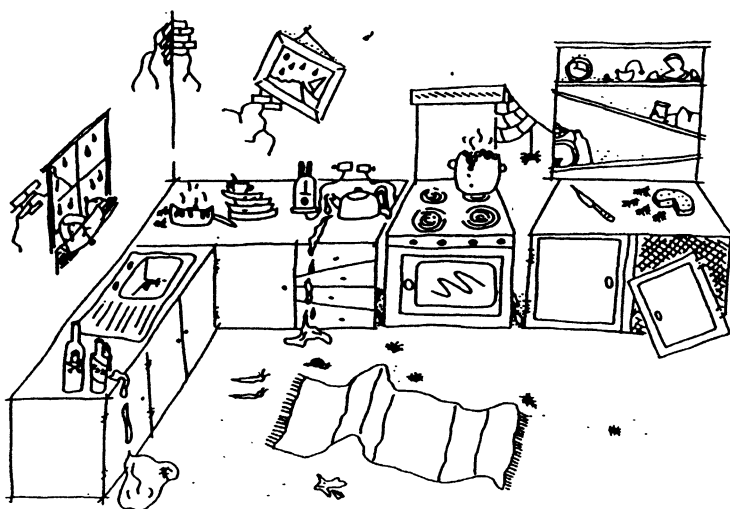


Figure 0.5 Source: Rachel Hacking.

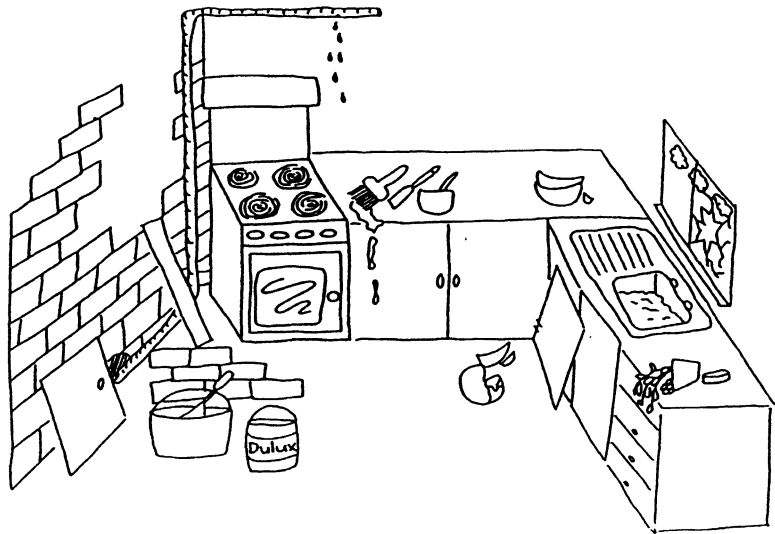


Figure 0.6 Source: Rachel Hacking.

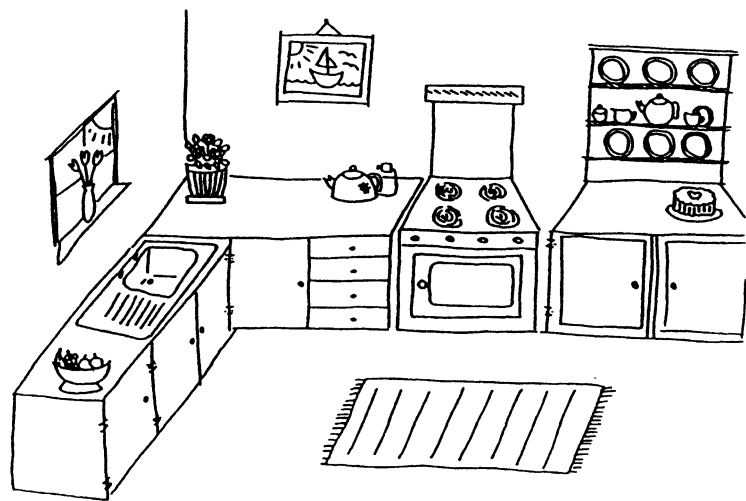


Figure 0.7 Source: Rachel Hacking.